without being able to distinguish the position of the stars. On the top of the wood-cut in the *Monthly Notices*, No. 5, is written "*Position des Satellites:*" these words are not mine, for I cannot believe that they are all satellites. In the possibility that some of these luminous points could perhaps be the result of optical illusion or sickness of my eyes, I tried the same kind of observation (by passages) with some brilliant stars, like *Altair* and *Antares*, but I could see nothing like that I perceived on *Sirius*; and so, having done all that is in my power, with an object-glass of only 4 inches (English measure) or 46 lines, to snatch such minute light points, I leave it to more skilful astronomers to do the rest; for a powerful telescope with clock-work and the star hidden behind a large plate would show this wondrous system. The distance of the star D I tried to measure on the moment of its discovery, and it might be distant east from *Sirius* $3^s{\cdot}60$ or $3^s{\cdot}80$ in Right Ascension. I found also another fainter and distant star (perhaps $10''$) in the vicinity, and on the east of the star D, which I announced in my first letter to Mr. Peters, on the 8th of March. On writing this, I received the news of Rev. Mr. Dawes' observation of the star D. It is, indeed, the most conspicuous of all by its *distant position*; but I think that the two nearest ones are more brilliant, and seem to be bodies of great importance relative to *Sirius*.

*Paris, le 5 Juin*, 1863.

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## *On the Discordance between the Results for Zenith Distances obtained by direct Observation and those obtained by Observation by Reflexion from the Surface of Quicksilver.*

By G. B. Airy, Esq., Astronomer Royal. (Abstract.)

After explaining the manner in which the discordance in question is practically taken account of at the Greenwich Observatory, the author remarks that, in preparing for the formation of the new Seven-Year Greenwich Catalogue of Stars, it appeared to him desirable to examine carefully the results of the reductions (extending over a quarter of a century) which have been conducted on the principle described, with the view of ascertaining whether the Direct Observations ought to be used alone, or whether the Reflexion Observations ought to be used alone, or whether the mean of them ought to be used, as has been done in the Annual Reductions; and that the object of the Memoir is to give the result of this inquiry. It appears that neither the D observations nor the R observations can be adopted, without correction, as sufficiently accurate, but that the D observations corrected by $\frac{1}{2}$ (R — D), or still better by $\frac{3}{5}$ (R — D), give accordant values

of co-latitude, and may be received as perfectly accurate; and it is inferred that the correction $\frac{1}{2}(R-D)$ or $\frac{3}{5}(R-D)$ is a real correction, founded on some physical cause. There is no room for explanation from defects of the instruments. A suggestion is, however, offered as to the nature of the physical cause of R — D, viz. from a comparison of the results for three different periods, for which different shutter-openings were used in the observing-room, it appears to be probable that the origin of the discordance expressed by R — D lies in some conformation of the warmer and cooler strata of the atmosphere in the immediate neighbourhood of the Instrument. The concluding paragraphs of the Memoir are as follows:—

"It is to me a matter of great satisfaction to be assured that the enormous number of observations made at Greenwich from 1836 have been reduced on principles which are fundamentally correct, and which scarcely leave appreciable error. But I think that the investigation leads to inferences of a broader and still more important kind. It appears probable that no co-latitude of an Observatory, where direct observations alone are used, is certain to a quarter of a second, and no north polar distance of stars at 70° or 80° from the pole is certain to half a second. I know no method of removing this doubt but the introduction of reflexion observations of stars on both sides of the zenith.

"I ought not to close this paper without acknowledging that my first ideas of connecting the discordance of direct and reflexion observations with the circumstances of the observing room, were derived from a conversation with Mr. Faye, when I had the pleasure of seeing him at Greenwich, on the occasion of determining the difference of longitude between Paris and Greenwich in 1854. At the time of reading his valuable paper in the *Comptes Rendus*, 1850, September 16, there was no critical evidence as to the cause of the discordance; and even in 1854, when the observations as far as 1852 were thoroughly reduced, there was not a sufficient amount of evidence under the new circumstances of the Instrument to enable me to form a decided opinion. It is only with the accumulation of evidence in later years, exhibiting results so consistent among themselves, but so distinctly opposed to those of former years, that the possibility has presented itself of pronouncing with certainty on the general origin of the discordance, and on the way in which, astronomically, it ought to be treated."